We would like to express our sincere gratitude to both reviewers and the editor for their insightful and constructive comments. Your detailed feedback has been invaluable in identifying key areas for improvement and will significantly enhance the structure and impact of our paper. We greatly appreciate the time and expertise you dedicated to reviewing our work.

We have implemented several changes in our revised version, including a more focused data analysis, a new structure, and extended discussions on the points raised in both reviews.

The main changes in our manuscript can be summarized as follows:

The areas of investigation were expanded by analyzing also smaller regions, using a higher spatial resolution and putting more emphasis on coastal processes.

We also extended the target variable (lead frequency) by specific polynya frequencies using a published data set to account for potential deficits of the leads-only data set in representing coastal process and to assess potential benefits from using a combined data set.

In the predictors list, we removed 2m air temperature and sea-ice concentration as they mostly represent proxies for other predictors and do not really contribute to explaining individual forcings for lead formation.

Finally, structure and analysis have changed substantially in the current version, where we tried to account for all of the reviewer's comments and suggestions.

Response to Referee 2 Comments

We show referee comments in black text, our response in blue and changes inserted to the manuscript are put in *blue italics*.

The manuscript presents a novel approach to determine the factors controlling the winter lead frequency in the Southern Ocean from a recent satellite product using random forest regression with permutation importance analysis. Based on this analysis, the authors identify the main predictors for lead frequency for the whole Southern Ocean and separately for the different basins, and discuss the associated mechanisms. This provides very useful information and the study is thus of great interest. However, additional information would be required before publication, in particular in the methodology and the impact of some of the modelling choices, as detailed below.

Your comments are valuable and will help improve the quality of the study. We have carefully considered and addressed each of your points.

General points

1. The manuscript should explain more explicitly the difference between lead frequency and ice concentration. The introduction discusses in general the impact of leads and opening within the ice pack, including studies that use ice concentration (from satellite products and models) as a measure of the surface occupied by leads. Naively, one could have expected that lead frequency and ice concentration would have been related. However, this does not seem to be the case, except in a few regions (e.g. Table 1). That would be useful if the authors explain why this naïve view is not valid.

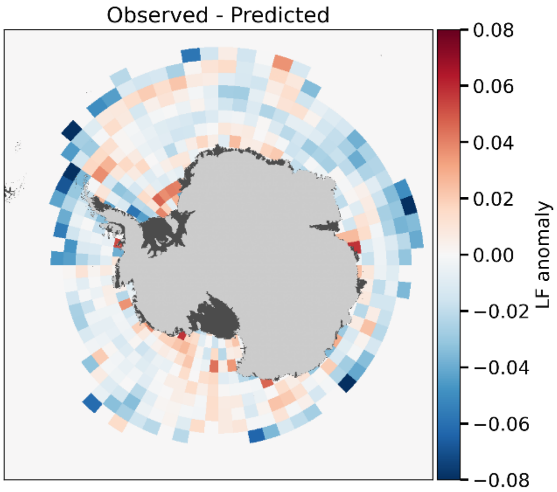
It is correct. Sea-ice concentration during wintertime is actually determined by the presence of leads. The problem is that operational SIC products (mostly passive-microwave) are generally too coarse to resolve leads themselves. In the revised manuscript we have therefore removed SIC as a predictor because it does not represent a physical driver for lead formation but rather an indirect proxy for their presence.

2. It may be hard to evaluate the model performance from the absolute numbers that are provided. Would it be possible to compare the model performance to the one of a much simpler model, assuming for instance a constant lead frequency in each basin or a climatological lead frequency at each point ?

   We appreciate this suggestion. While we have not introduced an explicit climatological baseline model in the revised manuscript, we have now added a coastal analysis section (Sects. 3.5 and 3.6) on a finer 10 × 10 km^2 scale that explicitly addresses the coastal basins, and it also compare results for LF (leads + coastal polynyas) with LFWP, where the polynya dataset is omitted. This section shows that the RF model captures variability beyond what a simple climatological field could explain, especially in coastal and shelf-break areas.

3. The regions where the differences between observed and predicted lead frequency are the largest are not easy to find from figure 1. From figure 2, it is clear that the predictions are biased for large values of the lead frequency but we cannot see on Figure 1 to which region this corresponds. Would it be possible to plot the difference between observed and predicted lead frequency in Figure 1 in addition to the absolute values ? This would facilitate the discussion. Additionally, it is mentioned several times in the manuscript that large biases are present close to topographic gradients, in bathymetrically-controlled and coastal zone, at the shelf break, etc. but this is not explicitly shown. Maybe a difference in Fig. 1 will help identifying those zones but an additional diagnostic may be needed to see it.

   We have generated the difference map (Observed - Predicted LF), which clearly shows that the largest underpredictions are concentrated along the Antarctic coastal margins, particularly in the Weddell Sea, Prydz Bay, and Ross Sea coastal zones, while the offshore pack ice shows modest overprediction. However, we have not added this as a third panel to Figure 1, because the spatial bias pattern is already addressed quantitatively through the dedicated coastal analysis in Sect. 3.5 (nine coastal boxes, Fig. 8) and the distance-from-coast analysis (Sect. 3.6), which together provide a more systematic characterization of where model skill is highest and lowest.



4. The random forest model is based on monthly mean values at a relatively coarse resolution (2° latitude × 5° longitude grid). This can be justified to reduce the amount of data that has to be processed. However, lead opening and closing could occur at shorter timescales and the link with some atmospheric variables could be obscured by monthly averages. For instance, we can imagine that the link between horizontal wind divergence and lead opening is very strong during the passage of a storm but weak at lower frequency. If possible, repeating some of the diagnostics (like Figure 2 and maybe Figure 5 if possible) at two different resolutions would allow testing this. If it is too time demanding, at least a discussion of the possible impacts of the resolution would be needed.

   We agree that using monthly means on a 2° × 5° grid can obscure part of the event-scale link between atmospheric forcing (e.g. storm-driven divergence) and lead opening. To partially address this, we emphasize that the coastal analysis is carried out on a finer 10 × 10 km^2 grid of the LF data and focuses on key coastal boxes (Sects. 3.5 and 3.6, Figs. 7–10). A full repetition of the RF diagnostics at higher temporal or event-scale resolution does not allow for a high resolution of predictors and is also beyond the scope of this study. We now explicitly highlight RF experiments using higher-resolution, event-scale forcing fields, including climate modes, as a priority for future work.

   "Understanding the connection between lead variability, coastal polynya activity, short-lived atmospheric events, and slow, pan-Antarctic scale climate shifts including climate modes therefore remains an important avenue for future work, particularly in the context of the variability observed in Antarctic sea ice since 2016."

5. The contribution of ocean processes to explain the lead frequency is relatively limited but only one oceanic variable is used (ocean current). Why not using more oceanic variables (like for instance the ocean current divergence – equivalent for the ocean to wind divergence) ? Eddy kinetic energy could also be a good candidate as the authors mention the role of the eddies in the discussion and eddy kinetic energy can be large in the region of rough topography and at the shelf break, regions where the model performance is apparently low. Another advantage is that, in contrast to individual eddy that are at a smaller scale than the one used in the model, eddy kinetic energy can be interpolated at the scale of the model. Alternatively, if bathymetry is so important, it could also be a good

predictor. This is maybe too much additional work for the current manuscript but the possibility that the ocean role is underestimated due to the limited number of variables used could be mentioned.

We appreciate this suggestion. We note that some aspects of eddy-driven deformation are already implicitly reflected in our ice-kinematic predictors, in particular ice divergence and ice velocity, which respond to mesoscale ocean features where sea-ice is sufficiently mobile. Nevertheless, we highlight the inclusion of oceanic predictors such as ocean current divergence, and eddy kinetic energy as priorities for future work in Discussion:

"Additional oceanic predictors, such as ocean current divergence and eddy kinetic energy, represent an important objective for future study, particularly in regions of rough topography and at shelf breaks where the model currently shows comparatively lower skill."

6. The added value of incremental predictor contribution (3.2) compared to the relative importance (3.3) is not clear. Is the relative contribution in the incremental method depending on the order in which the variables are added (while it is not the case for the relative contribution) ? Are they elements that are clear in 3.2 and not in 3.3 ? If this is the case, this should be more clearly highlighted or alternatively section 3.2 could be suppressed or put in supplement.

   We have substantially revised Sect. 3.2 to be more concise and to clearly articulate what it contributes beyond Sect. 3.3. The two analyses serve distinct and complementary purposes: permutation importance (Sect. 3.3) quantifies the independent contribution of each predictor within the fully specified model, while the incremental analysis (Sect. 3.2) reveals the marginal gain from sequentially adding predictors.

Specific points.

1. Line 32. The formulation can be interpreted as if ice divergence was an atmospheric process only.

   Corrected.

2. Lines 73-83. The introduction is nice and reviews well the different processes. However, the last paragraphs of the introduction are mainly a description of the work that has been done while the specific motivation of the study is not yet clear and this could be developed more at this stage.

   We thank the reviewer for this observation. In the revised version, the introduction is now more concise, and we explicitly highlight the knowledge gap and the specific objectives of our work, which we hope makes the motivation of the study clearer to the reader.

   We added: *"While several studies have described lead distributions and temporal variability in both polar regions, a comprehensive quantitative framework for identifying and ranking the physical drivers, and quantifying the relative importance of atmospheric, oceanic, and ice-dynamic controls on Southern Ocean leads and coastal polynyas is still lacking. This knowledge gap limits the representation of lead-coastal polynya related* *processes in coupled climate models and limits our capacity to assess how future changes in atmospheric or oceanic forcing might influence lead dynamics and their feedback on the polar climate system."*

3. Line 144. If it is standard recommendation maybe adding a reference would be useful ?

   Done.

4. Line 257-258. The first sentence is very general and could be suppressed.

   Done.

5. Is the Section 3.4 adding new information or could it be moved to the supplement for instance ?

   We appreciate the reviewer for raising this point. In the revised manuscript, the original Sect. 3.4 (which presented the monthly model performance for June) has been removed entirely and replaced by new analysis Sect. 3.5 titled "Model performance in exemplary sub-regions" and further with Sect. 3.6 " Coastal influences".

6. Line 241. The discussion about whole number percentage is confusing while on Figure 5 and in most of the text the number are given with one decimal. Why not giving all the numbers with one decimal ? Same issue line 311.

   Done. All percentage values throughout the manuscript have been standardized to one decimal place.

7. It not clear what is shown on Figure 8 and what is meant by sample-by-sample comparison? Is it the actual lead frequency and the predicted lead frequency on the same x-point ? If this is the case, what is the added information compared to Figure 7?

   The original Fig. 8 showing the sample-by-sample comparison has been removed in the revised manuscript now. In its place, the new Fig. 8 presents scatterplots of LF (leads with polynyas) and LFWP (leads without Polynyas) observed versus predicted monthly mean lead frequency, providing a clear and directly interpretable assessment of model performance in each coastal region.

8. Lines 396-399: Is there a good way to test the hypothesis of 2m temperature acting as proxy for offshore winds? Maybe by testing the correlation between temperature and wind speed/direction or something along those lines?

   In the revised manuscript, this point is no longer relevant because we have removed 2 m air temperature from the predictors. Instead, we now use the zonal u and meridional v wind components. These wind components act as direct physical drivers of lead formation, so the earlier concern about T2m being only an indirect proxy no longer applies.

9. Line 374. The previous paragraph states that 2m air temperature is the main driver. How this finding is it 'consistent with earlier work' which shows that Antarctic leads tend to occur in area influenced by bathymetric steering ? Maybe 'this' refers to something else ?

   This apparent inconsistency arose from the previous version of the manuscript where T2m was the top predictor. With T2m removed from the predictor set in the revised manuscript, the zonal u wind now emerges as the primary pan-Antarctic driver of lead formation. The relevant passage has been revised accordingly and the original inconsistency no longer exists.

10. Line 381. That would be useful to give the coordinates of Maud Rise and Gunnerus Ridge (or to show them on a figure) and maybe show the biases of the model for those points ?

    The approximate coordinates of Maud Rise (~66°S, 3°E) and Gunnerus Ridge (~67°S, 34°E) have been added in the revised text where these features are first mentioned in Sect. 1.

11. Line 471. The link with 'bathymetric contrasts' is not clear here.

    We thank the reviewer for noting the unlink reference. In the revised manuscript, we have updated the Discussion to reflect the new analyses, and this sentence has been rephrased so that the ambiguous link to "bathymetric contrasts" is removed.

12. Lines 475-478: If r=0.73 shows comparatively higher model skill, what is it in comparison to? And, if the Ross sea has many events that are on sub-monthly scales that the model can't account for, then shouldn't it result in lower model skill?

    We acknowledge that the original text was misleading in describing the Ross Sea as showing "comparatively higher model skill". In the revised manuscript, the Ross Sea correlation of r = 0.73 is now described in the context of the full range of regional correlations (0.63–0.82), noting that the Ross Sea shows moderate skill comparable to the Weddell Sea, rather than being described as comparatively higher.

13. Line 506. Is this indirect dynamical effect due to the resolution of the reanalysis (and the spatial resolution of the model) that is not able to reproduce the winds and their divergence at the scale that matters for leads opening ? (see also the general point 4 above).

    This point is no longer directly relevant in the revised manuscript, as 2 m air temperature has been replaced by the directional wind components, removing the need to interpret an indirect dynamical effect through temperature.